

Proxy types, taxonomic discretion, and taxonomic progress: a response to Löbl et al.

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We were glad to see the Löbl et al. (this issue) response to our recent paper on the application of Article 73.1.4, in which we put forward that special circumstances can justify the naming of a new species using a photograph as a proxy for a lost type specimen (Marshall & Evenhuis, 2015). We framed that position against a simple and specific example in the hopes of drawing attention to this issue, which we expect to be of increasing interest and importance in the coming years. The thoughtful counterpoint provided by Löbl and colleagues is exactly the kind of discussion we had hoped to stimulate, and we welcome the opportunity to address their criticisms of our point of view.

Their most substantive, and most easily dismissed, criticism is that the formal naming of a species in the absence of a type specimen questions the scientific foundation of taxonomy, and ‘*is in the domain of belief*’ rather than science. This suggests a rather peculiar line between ‘science’ and ‘non science’. The description and naming of a new species, whether a formal binominal is used or not, and whether or not the type is lost, represents a set of testable hypotheses about character state distribution and thus about the reality of the species described. A description that points to attributes visible in a photo is no different in this regard than a description that points to attributes of a physical type specimen. A description based on character states visible on a compression fossil, a description based on character states captured using photography, and a description based on character states recorded by direct observation of a pinned specimen or permanent slide mount all make similar predictions (testable hypotheses) about the distribution of those character states in nature. In the normal course of events those predictions are tested when additional specimens are observed, not by re-examination of the original type specimen.

Although there is no case to be made for distinguishing science from non-science by the presence or absence of a designated type or voucher, we heartily endorse the recommendation that all scientists dealing with species designate vouchers. We frequently remind students in ecology and other disciplines that the value of their work could be greatly diminished if in the future the identities of their research organisms is questioned and there are no vouchers to examine. This can be a problem if the identification of the research organisms is suspect, or if a named research organism turns out to be a complex of taxa. Taxonomists have long relied on vouchers, in the form of holotypes, to guard against the possibility that their written descriptions prove inadequate to separate their species from others, newly discovered or not. And indeed, we have spent large chunks of our careers studying older holotypes of species unrecognizable on the basis of published descriptions. A welcome development in recent years has been the widespread availability

of photographs of types, often of adequate quality to save the expense, risk and difficulty of borrowing a type specimen for direct examination. A more ‘game-changing’ development is the increasingly common practice of including detailed digital images of specimens, or even living individuals, as a routine part of new descriptions. Such practices minimize, but do not eliminate, the possibility that recourse to the type specimen might be needed in the future. The standard practice of designating types, or vouchers, thus remains a good one although we maintain that the lack of a type specimen should not stand in the way of providing a name for a thoroughly substantiated and well-illustrated species.

Löbl et al. maintain that a photograph is not acceptable as a surrogate for a lost type specimen because ‘*any photograph can easily be modified or misinterpreted*’. Let us look at these two problems in turn, starting with ‘modified’. The possibility that someone could deliberately clutter the literature with a fake photo, fake fossil or fake specimen is a real one. The Piltdown Man story is a famous example of this, as is the more recent forgery of ‘*Archaeoraptor liaoningensis*’. Obviously, this possibility is not unique to photography. We can all point to bad descriptions, misleading illustrations, and mangled/fragmentary/slide-mounted type specimens that border on ‘fake’, and it would be no harder to describe a specimen based on a deliberate chimera than on a deliberately modified photograph. We have encountered accidental chimeric type specimens (mismatched genitalia and bodies) and suspect that such errors are not uncommon. Minor adjustments to images (such as brightness and contrast) are far less ‘modification’ than the fading or discoloration commonly seen in physical specimens, but we concede that deposition of an original raw digital image file is a good idea. The essential point here is that if someone wants to cheat, a requirement for a type specimen is hardly a barrier. But deliberate cheating is so unusual we do not consider it to be a problem.

Now, let’s move on to ‘misinterpretation’. If a photograph or set of photographs is good enough for a taxonomist to use it as a proxy type, why would it be more subject to misinterpretation than a specimen? A good photo of a living micropezid fly, for example, shows true colour, true posture, and exposed membranous structures not visible (or subject to misinterpretation) on a pinned specimen. There are no grounds to suggest that a good photo is more likely to be misinterpreted than a specimen, and in fact a good photo of a living specimen is far less open to misinterpretation than a permanent slide mount, a compression fossil, a shrivelled pinned specimen, or a fragmentary specimen (all of which are routine and acceptable as holotypes).

The ‘*concrete case of complications and confusions*’ given by Löbl et al. is an interesting story and a familiar one to any taxonomist, but with little bearing on the issue under debate. Every time we examine a previously named species in the course of a revision there is a possibility that it will turn out to be two or more species, and that the additional species will be undescribed. A normal part of that process is to figure out which of the multiple species corresponds with the original name. Robb et al. (2013, cited in Löbl et al. [this issue]) recognized that there were two species under the name *Strix butleri* (Hume), and that one of them must therefore be new. Unfortunately, they misidentified the new species as the one corresponding to the original name even though there was a type specimen corresponding to that 1878 name, and this led them to coin a new name for *S. butleri*. Kirwan et al. (2015, cited in Löbl et al. [this issue]) discovered this error by examining the type specimen of *S. butleri*, and showed that Robb et al. picked the wrong one of those two species to describe as new. The species described by Robb et al. was therefore a junior synonym. This is all routine stuff for any taxonomist, and led to

hypotheses later tested by obtaining and sequencing new material. The only twist to the tale is that Robb et al. (2013) based their description on multiple photos and recordings rather than a dead bird, a justifiable decision under the circumstances.

Löbl et al. allege from the above example that ‘the absence of specimens, as in the new species of *Marleyimyia* described by Marshall & Evenhuis (2015), prevents the discovery of additional characters and is foreseen as a source of future problems’. In fact, such ‘problems’ can only arise once new specimens have been collected and studied for additional characters. It is possible that such new specimens will include two species externally identical to *Marleyimyia xylocopae* Marshall & Evenhuis. This is little different from a routine problem created when an insect species is described from females and it is later found that it is one of two species differing only in male terminalia. When an existing female type could correspond to either species, a pragmatic choice is usually made based on collection locality. *Marleyimyia* would be an easier case to resolve since the type is lost and the Code specifically allows for the designation of a neotype, which would neatly solve the problem of the two newly discovered externally identical species.

The phrase ‘*prevents the discovery of additional characters*’ is worthy of further comment since it is of course impossible to sequence or dissect a photograph. We agree that this is a good reason to make every effort to obtain a type specimen or type series, but to argue that new species must be based on types available for future extraction of molecular data or study of internal morphology would be a slippery slope indeed. This would obviously present a problem for the many taxa routinely described from permanent slide mounts, and of course for taxa described from compression fossils or amber, but it goes much beyond that. What about specimens treated in such a way that the DNA is destroyed? Flies collected into pan traps often sit in water for days, and then are stored in 70% alcohol for long periods before being soaked in ethyl acetate or other chemicals in preparation for mounting . . . are such specimens to be disallowed as types if they cannot be sequenced? What about very old specimens, or damaged specimens, or specimens that cannot be dissected? Of course it is ideal to have long series, both sexes, illustrations of genitalia, fresh specimens, photographs, behavioral data, sequence data and so on, but in practice taxonomists must work with what they have.

In the next paragraph Löbl et al. grant that the discovery of ‘*unexpected, spectacular new species is doubtlessly an event that requires publication*’, that ‘*such publications are desirable, and nothing in the world impedes authors to write about them*’, but that taxonomists should *denote such taxa informally*, and that ‘*conservationists, ecologists and others may use published information about unnamed taxa just as well as if they would have been denoted by available binominal*’. We do not entirely agree that the discovery of spectacular new species requires publication, and in fact we feel quite strongly that the description of a new species is only a contribution to science if it is done in the context of a meaningful group and if the species is unequivocally distinguishable from every member of that group over a significant geographic area. Papers describing out-of-context, single new species in unrevised taxa are often a burden on taxonomy, with or without types. But if a species warrants description and if enough information is available to adequately describe and illustrate the species, we consider it pointless to name the species outside the purview of the Code. We do indeed want ‘conservationists, ecologists and others’ to use our taxonomies, and that is one reason we want to work within a system for communication using names that are unique, universal, stable, and reflective of relationships. If a species is worth naming, it is worth naming using the Rules of Nomenclature.

Now, let us examine, point-by-point, Löbl's criticism of our discussion of the future importance of digital images.

'Taxonomy might be threatened because of the increasing power and availability of digital photography, improving one's chances of quickly capturing images of animals, without carefully studying the animal themselves'. This point is entirely backwards! Macrophotography demands hours of observation and tremendous patience, as opposed to collecting which is increasingly done with Malaise traps and other mass collection devices. Photos of living individuals, perhaps showing behaviour, microhabitat or soft structures, are a tremendous adjunct to any species description. The thought that this 'threatens' taxonomy is not well-founded.

'The notion of 'rarity' has meaning when sizes of populations are known. This is certainly not the case in Marleyimyia, as in the bulk of other animal species. Such 'rarity' only points to a lack of knowledge.' It is true that the term 'rare' is often used loosely for 'rarely collected' or 'apparently rare', and *Marleyimyia* is both. All evidence suggests that *Marleyimyia* really is a rare taxon, but this has no bearing on the question at hand.

'Examination of physical specimens may reveal biological characters that cannot be captured solely by photography, precluding adequate comparison with closely related taxa.' Also true. But so is the converse . . . examination of photographs may reveal biological characters not available on physical specimens. We really should have both, but in the real world we make decisions based on what is practical.

'it is not entirely clear if a neotype can be proposed when a name was established in the absence of a physical type' The Code glossary defines 'neotype' as 'The single specimen designated as the name-bearing type of a nominal species or subspecies where there is a need to define the nominal taxon objectively and no name-bearing type is believed to be extant.' In the case of *Marleyimyia*, we state that the type was lost during collection. We feel that statement falls within the definition of 'no name bearing type is believed to be extant'. This would no doubt be the case for other situations where photography alone was used to depict a type specimen.

'every hour the number of invertebrates dying naturally or accidentally killed by humans, not counting purposeful destruction of unique habitats, probably exceeds that of all the specimens ever collected for scientific purposes around the world. Nevertheless, the legislations adopted as a consequence of inadequate information and side-effects of the Nagoya Protocol have added notable additional difficulties in sampling specimens. By bureaucratic requirements and uncertainties they discourage field research, and hamper the increase of knowledge about forms of life that evolved and occur on our planet.' We agree wholeheartedly with this entire paragraph, as the impact of restrictive legislation on the progress of taxonomy is a serious problem. It is difficult to get permission for narrowly targeted collecting and essentially impossible to get permission to do traditional 'general' collecting in most countries, and thus general collection growth has stalled. This will have a great impact on future taxonomists looking for new material and distributional records. Some of that need will be met by the growth of digital image collections, a reality we will have to come to grips with whether we like it or not. And if Article 73.1.4 allows a taxonomist to complete a work by utilizing images of specimens from an area from which it is impractical to obtain physical specimens, then this is a good thing.

'publications such as those of Marshall & Evenhuis (2015) or Minter et al. (2014) may also stimulate non-experts to describe new species based on photographed specimens.' This is the objection most often raised to the practice of using photographs as

proxy specimens. There is a fear that non-experts, or (worse yet) experts who already have the bad habit of scratching their 'mihi itch' (see Evenhuis 2008) with out-of-context or opportunistic descriptions, will clutter the literature with dubious new species based on photos. We think this is an unfounded fear and believe quite the opposite will result. Since the intrinsic transparency of submitted papers with descriptions that are based all or partly on photos will lead to an increased scrutiny by both editors and reviewers, it should instead result in a drop in published nuisance descriptions. On the other hand, Article 73.1.4, used responsibly, will give good taxonomists one more tool to ensure that their publications are complete and their taxa are appropriately recognizable.

'The unrestricted use of this article of the Code may affect institutions, which are already facing difficulties being depended on sponsors with a poor understanding of the need in keeping physical specimens. With dwindling financial support, natural history collections will encounter further difficulties if decision-makers consider scanning specimens to be an acceptable proxy for physical specimens.' This is a worrisome point. We have had similar concerns about the possibility of new restrictions on collecting due to a misconception that specimens are no longer needed. It would certainly be ironic if this were the case, since we are strong proponents of insect collections (i.e. Marshall, 1992, Wiggins et al., 1991, Brunke & Marshall, 2011) and vocal critics of restrictions on insect collecting (Marshall, 1995). We trust that anyone with enough interest in the subject to make that sort of decision will read beyond the title of our paper, and see a strong case for collections and collecting.

In summary, consideration of the arguments against the judicious use of photo-based descriptions shows them to be ill founded although well intentioned. We remain convinced that the ICZN should continue to allow taxonomists to use their discretion with regard to the material foundations for their new taxon descriptions. Type specimens are an expectation and a default, but if unusual circumstances dictate the recognition of a new species based on photographs of a lost type there is no good reason to prohibit this practice. And if new taxa are going to be recognized and published on the basis of photos of lost specimens, then they should be named according to the rules of nomenclature.

NB: The views expressed here by the junior author are his own and do not reflect those of the Commission or its Commissioners.

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